

Memorandum

To : T.H.Milby, M.D.

Date : October 24, 1972

Subject: "Interim Re-Entry
Periods" Recommended
by Task Group on
Occupational Exposures
to PesticidesPERSONAL
AND
CONFIDENTIAL

From : Henry Anderson

You have told me, a number of times, that your reputation is riding heavily on everything which emanates from the Task Group. From this standpoint, to say nothing of concern for farm worker safety and other considerations, I think you should be aware of some aspects of the "Interim Re-entry Periods" which the Task Group has recommended, and which, as someone said at the last meeting, will now be duly "bubber stamped" by the Federal Working Group, Council on Environmental Quality, and NIOSH, on their way to final review by the Department of Labor.

At least three of the five subcommittee reports -- all of them accepted without a single change -- were wholly based on demonstrably fallacious assumptions. The most charitable construction that can be placed on this is that subcommittee members were not in possession of the true facts; but in some cases it is difficult to conclude anything but that the relevant facts were deliberately suppressed.

1. Apples. I quote from John McCarthy's report on October 18: "...from these studies in Washington...it was measured that a certain amount of (parathion) was deposited on the skin of a thinner during his normal activity... The assumption was made that 3% of that was absorbed... ...there would be, using a maximum figure of potential, 2.93 milligrams of parathion per day that could get into the body, making those assumptions which I just indicated." (Emphasis added.)¹

¹ Page 25, lines 2-13.

Everything rested on that assumption of 3% absorption -- not only the parathion waiting period of 3 days, but the periods for all 15 other registered compounds, which were without exception either 1 day or 3 days. As McCarthy later stated, "We essentially ignored any persistence information..."

Now, the truth is that the 3% assumption is simply nonsense. Whatever it might have been based upon (apparently an estimate by Wolfe ten years ago) has long since been superceded by Maibach's work. He reports 8.6% absorption of parathion from the forearm, 11.8% through the palm, 36.3% from the forehead, etc. Even if one uses the lowest of these figures, the Wolfe-McCarthy recommendations are all wrong by a factor of 300%. That is, re-entry periods should be three times as long as they recommend in order to hold apple workers' exposure to the threshold limit value of 3 milligrams ^{of parathion} per day.

If one is really serious about worker safety, one has to multiply by more than that, for all exposure obviously doesn't come through the site of most resistance (the forearm). At least as much comes through areas of far greater permeability, such as the forehead. In logic, their values ought to be multiplied by factors of at least five -- and even that would leave many of them still far short of the waiting periods required for protection of consumers: 15 days for phosphamidon and trit^hon, for example, rather than 30.

The most curious part of all is that McCarthy and Wolfe were in possession of information about Dr. Maibach's work. They were present, as I recall, at the April 6 meeting in Berkeley, at which Dr. Maibach made an oral presentation, and everybody received in the mail a summarization of Maibach's work -- including a specific reference to absorption of parathion through the forearm.

2. Citrus.

Quoting again from the transcript of the October 18 meeting, Gunther said

"...at intervals after application varying from a few days to several weeks, with 10 or 12 of these materials, ~~from xxx~~...up to 80% of the dislodgeable residues on fruit can be removed by washing procedures. (pp. 1-2) From present information, we feel that...a thorough coverage spray with dilute detergent solution would be adequate to give essentially clean foliage-- essentially free of dislodgeable residues. For example, in some of these chemicals that have been studied, 45% total residues on and in leaves can be washed off within 10 days after application. This 45% represents the dislodgeable residues as a fraction of the total residue present at that time. (p. 4.) ...we have adopted a device that we know...will drop these dislodgeable residues drastically before the workers reenter the groves. ... With Morestan we can wash 75% of dislodgeable residues off after 26 days both for fruit and foliage, and with Delnav we have 18% versus 45% on the foliage -- correction, 20% on the fruit -- and about the same time interval. By analogy, we are concluding that we are taking the safest possible course in this recommendation. (p. 6.) ...washing, based on existing information, is probably going to be more than enough in many instances. (p. 48.)

[Emphasis added.]

I have not seen the Morestan data, but in any case it is irrelevant on at least two counts: it is not registered for use on citrus anywhere in the U.S.; it is scarcely more toxic than milk (3,000 mg/kg) compared to Delnav (43 mg/kg). So Gunther's entire case, really, rests on Delnav, and here I have seen the data. They are as follows.

| Days between spraying and washing | Leaf surfaces ¹ | | Fruit rind ² | |
|-----------------------------------|----------------------------|---------------|-------------------------|---------------|
| | Before washing | After washing | Before washing | After washing |
| 4 | 114 | 0.6 | 3.7 | 3.3 |
| 10 | 0.8 | 0.5 | 4.2 | 4.5 |
| 38 | 1.0 | 0.7 | 4.6 | 3.5 |
| 46 | 0.7 | 0.5 | 4.0 | 3.4 |

NOTE: Residues represent average of three replicates.

1: Micrograms per square centimeter

2: Parts per million

To any reasonable, open-minded person, these figures prove precisely the opposite of Gunther's claims. Far from reducing residues "drastically," and rendering foliage "essentially free of dislodgeable residues," washing has astonishingly little effect on Delnav. After 10 days, there was more residue on Gunther's test fruit following washing than before washing -- which, of course, is due to sampling variation, but is a fair indication of how persistent this compound really is, dust or no dust, and how inept a method washing really is.

It is perhaps even more instructive to translate the dislodgeable leaf residues from $\mu\text{g}/\text{cm}^2$ to parts per million. (I have serious reservations about expressing residues in the former terms. They are significant only to a single decimal, and the numbers are so small they create an impression of innocuousness when levels are anything but innocuous.) The most common rule of thumb is that $1 \mu\text{g}/\text{cm}^2$ is equivalent to 70-80 ppm. Therefore, Gunther's data show that after 10 days (his recommended waiting period), there were approximately 40 ppm of available Delnav even after washing. And 46 days after application of Delnav, there were still 40 ppm available in dislodgeable residues after washing.

What conclusions can any knowledgeable, honest observer draw? (1) 40 ppm of a compound as toxic as Delnav is not tolerable in terms of worker safety; (2) no amount of washing -- even in combination with an extended waiting period -- is able to bring Delnav below this level. Ergo, Gunther's recommendations with respect to Delnav are utterly unacceptable; and all his other recommendations are equally untenable, since they were all extrapolated from what he claimed were his good results from experimental washing of Delnav.

I cannot resist pointing out, once again, that Gunther was well aware of the above data. They are his own data. They are from an article he co-authored with Westlake and Carman, entitled "Worker Reentry Research: Dioxathion (Delnav) Residues on and in Orange Fruits and Leaves, in Dislodgable Particulate Matter, and in the Soil Beneath Sprayed Trees." Copies of this article were distributed, apparently some time in September. One reached Louis Lykken, who happened to send it to Donald Mengle, and I happened to see Don's copy -- but not until the day after the Task Force meeting. Copies were never distributed to the Task Force members. Nor did Gunther, at any time, give the Task Force an accurate resumé.

Why? Why this suppression and distortion of evidence? How could Keith Long and John Davies -- and, for that matter, the whole Task Group sitting in plenary session -- have been taken in by it? I think some very unpleasant questions must ultimately be faced, but that is beside the immediate point. The immediate question is what to do about a set of recommendations which rest wholly on false information. The great pity of it all is that, contrary to Gunther's repeated and unchallenged assertions that we have no good data for citrus, we have more good data on citrus -- epidemiological, foliage residue, controlled cholinesterase experimentation -- for more different compounds, than on any other crop, with the possible exception of grapes. Those data have been in the hands of the Task Group for months. Why a group of supposedly sophisticated scientists would have sat back and accepted, without a murmur, the statement that there were no data... Well, don't let me get started on that again.

3. Peaches. All the re-entry periods here, as you will recall, were based

on extrapolations from a study conducted not at harvest time (which is the only time there've been demonstrable problems) but three months earlier, at thinning time. There are many reasons why it is invalid to extrapolate even for the test compound, parathion (first application of the season; low dosage; weather unrepresentative; etc.), much less 15 unrelated compounds.

A second procedure by the peach subcommittee is almost equally dubious. An "anxiety index" was obtained by multiplying a crude persistence rating by a crude toxicity rating -- thus accentuating rather than minimizing the two forms of crudity. (As an instance of how crude the device was, phosdrin was assigned a toxicity index of 4, thus ranking it below parathion and Systox (even though it is more toxic than either, both orally and dermally), and lumping it with such as ethion (even though it is ~~over~~ ten times more toxic than ethion orally and nearly sixty times as toxic dermally).)

4. Grapes. At least, the peach subcommittee could argue, with some measure of justification, that there has been relatively little leaf residue work on peaches, and no controlled studies of human affect at harvest time. But, incredibly enough, the chairman of the grape subcommittee chose to extrapolate from a peach thinning study to all compounds and all job-activities on grapes -- even though excellent residue work had been done for five compounds on grapes, including some controlled studies of human affect *directed by himself!*

Although I ~~was~~ unaware, on October 18, of the extent to which Gunther was suppressing his own data, I was aware that Bailey was doing so, and I asked him why, in my most tactful way:

Anderson: It seems to me you are very modest in leaving out consideration of your own grape study... You found what could be very relevant information about residues of Guthion, etc., on grapes. ...you found quite high levels of

Guthion up to 32 days, yet you give it the same period of 14 days as you gave on peaches. I fail to follow that reasoning.

Bailey: ...We had in that instance of that test we combined Guthion and ethion and we applied 30 days before harvest and about 14 days later we came through and sprayed the same block of grapes with Zolone... And finally about 4 or 5 days before harvest another legal application of Dibrom was applied right over the same vineyard... What we don't know is what effect that one chemical had on the degradation of another and we really can't say yet whether one potentiated the other and therefore caused the more rapid or greater cholinesterase depression. (pp. 22-23.)

It is difficult to know whether Bailey was being ingenuous or disingenuous here. If one compound potentiates another, it is, in effect, breaking down the second more rapidly. If they are synergistic, they both break down more rapidly. As a pesticide safety expert, he should know this. Yet he was suggesting that in his grape study, the several compounds might somehow have extended each other's lives -- a phenomenon never observed, and, indeed, physically impossible unless the molecules of Compound B settled on top of ^{all} the molecules of Compound A by some miraculous chance, thus blocking access to heat, light, etc.

It is hard to believe Bailey was ignorant of these elementary truths, for at least two reasons. An earlier study of two of his test compounds -- namely, Chemagro's ~~Hexagone~~ study of ethion and Guthion on citrus -- found that "the rate of Guthion EC decay on citrus leaves was noticeably more rapid in the presence of ethion than in its absence" while "the rate of Guthion WP decay is unaffected by the presence of ethion WP." (In his grape study, Bailey used wettable powders.) I.e., not a scintilla of support for the novel theory that these two can somehow render each other more persistent.

Secondly, Bailey was well aware of degradation curves from his own study which proved that the decay of ethion and Guthion were not affected at all by the subsequent application of phosalone and naled. This analytical work was done

within our Community Study on Pesticides, and communicated to him both orally and in writing.

The question, once again, can hardly be shrugged aside: why did he reject his own hard scientific data, which pointed toward at least 30 days for Zolone and Guthion, and 21 days for ethion, etc., in favor of a totally preposterous, unscientific extrapolation which yielded a 14 day recommendation for Zolone and Guthion, 10 days for ethion, etc.? Well, once again I have my own conjectures, but this is not the time or place for that.

5. Tobacco. Since we never heard from this subcommittee, it is hard to comment on their procedures. I can only point out that in the six compounds where comparisons are possible, the Task Group has recommended one reentry period which is the same as the existing pre-harvest interval, and five which are shorter -- sometimes dramatically shorter. Three days instead of 21 for dimethoate, for example, and 4 instead of 15 for methyl parathion. When these recommendations become public, someone is absolutely certain to conclude that either EPA is beingridiculously over-protective of consumers or we are beingridiculously under-protective of workers.

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To lay it right on the line: the small inner group which set California's "Worker Safety Time Periods" has been able to get away with alibis about "no data" and "best scientific judgment" (although if you read a bit between the lines of my memo of October 3, 1972, you will see that these alibis could quite easily have been exploded if worker support groups had any sense). But the Task Group

cannot lean on that threadbare type of excuse. In the majority of cases, they had data readily available which they ignored, distorted, or suppressed. The single thread which binds this sorry story together is not "scientific judgment," but a consistent bias which selected only that evidence, no matter how far-fetched, which seemed to allow shorter re-entry periods than anyone had ever dared suggest before: shorter than label requirements, shorter than California's regulations which are already shorter than label requirements in nearly 80% of all cases.

To say the least, this is a ludicrous position for a Task Group which owes its very existence to the demonstrable fact that label requirements, far from often being too long, are/too short for workersafety. But the situation is worse than ludicrous for anyone on the Task Group who cares about his reputation as a scholar or a gentleman. All the evidence any intelligent observer needs to prove the bias of the Task Group's recommendations is in the public domain. Maibach's data which devastate the apple recommendations are readily available; Gunther's data which will crucify him and his orange recommendations are circulating freely in manuscript form, and will soon be published; Bailey's grape data and the analysis thereof have been widely distributed in our Community Study on Pesticides annual reports; etc.

The farm labor support groups are arrogant, pigheaded, impossible to work with. (I pleaded with them not to file that petition for "emergency re-entry periods" which got us into this mess; after they went ahead and filed it, I wrote them a letter so scalding you wouldn't believe it: "...the blood of farm workers will be on your heads...") But they are not stupid. If the Task Group

recommendations appear in the Federal Register, on or about December 1 as presently planned, in anything remotely resembling their present form, the farm labor groups are going to smell the bias instantly, they are going to marshal the readily available evidence which proves the bias, the falsification of data, and all the rest of it, and they are going to pillory not only the Task Group's numbers but the individuals who lent their names to those numbers.

I am going to do whatever is in my power to prevent that from happening -- not because I have any interest in salvaging the reputations of Wolfe, Gunther, and Bailey, ^{et al.} but because I have a consuming interest in salvaging a decent, defensible set of re-entry periods, and through them the health of a lot of farm workers who would be hurt by an indefensible set. I propose, first, to communicate with Jon May: my reasons for regarding the present recommendations as an almost unmitigated disaster; my suggestions for averting the gathering disaster by bringing out from hiding the good, usable data for citrus, grapes, etc. If review by NIOSH proves to be a rubber-stamp operation, I propose to communicate, next, with Dick Ronk and his group in OSHA. All of this very privately and discretely. I am not too optimistic about accomplishing much, if anything, in this way. Who am I to set myself against such internationally-renowned authorities as Francis Gunther, Harry Hays, John McCarthy, Blair Bailey, Homer Wolfe, and yourself? I suspect the only recommendations which will be significantly changed ~~will~~ during administrative review will be those for citrus-- and those for the wrong reasons: not because they were dishonestly derived, and would leave citrus workers in great peril, but because of the "inconvenience" of washing to growers. I anticipate that NIOSH-OSHA's way out will be to make no recommendations for citrus at all -- a bitter irony, since, as I said earlier, we

are actually in a better position to make solid regulations here than in any other crop, with grapes a close second.

If the Task Group recommendations are, as I fear, published in the Federal Register in substantially their present form, I shall be obliged to continue doing whatever I can to modify them in the direction of farm worker safety -- and beyond this point, my efforts will of necessity have to be public rather than behind the scenes. DOL anticipates a public hearing during the first two weeks of January. It will be in Washington, of course, and of course I won't be able to testify in person. I'll file a written statement, and because my nature is what it is, the statement will be a vigorous one. Naturally, I won't name names as I've done here with you, but if it ever comes to the point that anyone is put on a witness stand under oath -- and I think it well might -- there'll no longer be any secret about who was on the apple subcommittee, their industrial affiliations, etc., etc.

Let me end as I began. You have often told me that you are acutely aware how closely your personal reputation is tied to the accomplishments of the Task Group. I respect that. This memo is intended to warn you that your reputation is seriously threatened by a series of unbelievably ill-advised recommendations on "safe" waiting periods. If something is not done to deflect those recommendations, the Milby Task Group will ^{probably} be remembered by history only as the group, ostensibly concerned with worker safety, which issued proposals so hazardous they had to be enjoined by court order.

How you respond is up to you, but, speaking as your friend, it seems to me there are only a couple of really viable alternatives. You can use your influence with NIOSH and OSHA, during the forthcoming month of administrative review, to

get the more blatantly insupportable re-entry periods changed. The insiders may listen to you where they wouldn't listen to me, so there may be some hope in this approach.

If this fails, and the recommendations come up for public review in essentially their present form, it would seem that you may have to bite the bullet and testify against them: in effect, disassociating yourself from your own Task Group. It is said that a captain should go down with his ship -- but I hardly think that truism applies if the ship is rotten and deserves to sink.